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Dear Arthur,

I am sorry not to have replied more promptly to your letter dated 7th October, but this term has been rather hectic, as I have been lecturing both at Oxford and in London and dashing backwards and forwards between the two places!

Now the term has ended I have settled back to read your interesting paper on Correlations and Physical Locality.

But first let me explain my point about your 1977 proof that the Correlation Rule, together with Conservation and Fact leads to inconsistency. The assumption of locality comes in, as I see it because you do not allow the possibility that the value of $I \otimes B$ in the state $U(\phi \otimes \xi)$ depends on whether you are measuring A or $f(A)$ on the first system. If you did not assume locality you would have to write

$$[A \otimes I]_{B}^{U(\phi\otimes\xi)} = X_m \Rightarrow [I \otimes B]_{A}^{U(\phi\otimes\xi)} = Y_m \quad (1)$$

and also

$$[I \otimes B]_{F(A)}^{U(\phi\otimes\xi)} = Y_m \Rightarrow [F(A) \otimes I]_{B}^{U(\phi\otimes\xi)} = F(X_m) \quad (2)$$

where I use the notation $\left[I \otimes B\right]_A^{U(\phi \otimes \xi)}$ to indicate the value of $I \otimes B$ in the state $U(\phi \otimes \xi)$ when the apparatus is set to measure A on the other particle etc.

But from (1) and (2) I cannot deduce

$$\left[f(A) \otimes I\right]_B^{U(\phi \otimes \xi)} = f\left(\left[A \otimes I\right]_B^{U(\phi \otimes \xi)}\right)$$

simply because

$$\left[I \otimes B\right]_A^{U(\phi \otimes \xi)} = \gamma_m$$

$$\not\Rightarrow \left[I \otimes B\right]_{f(A)}^{U(\phi \otimes \xi)} = \gamma_m$$

this implication only goes through if we do not distinguish $\left[I \otimes B\right]_A^{U(\phi \otimes \xi)}$ from $\left[I \otimes B\right]_{f(A)}^{U(\phi \otimes \xi)}$ as in your 1977 paper, and this is what I meant by saying you proof assumed locality.

I would then want to argue that your proof of inconsistency is really a proof of nonlocality, if we decide to hang on to the plausibility of Correlation (which after all is a particular case of the extended Spectrum Rule for genuinely commeasurable observables, which you allowed in your 1974 Synthese paper).

Do let me know what you think about this.

Now let me make a few comments on your new paper:

p 19 In your discussion of responsible indeterminism, the reason that the probabilities for each λ are constrained by (CH) is that $p(ST, \lambda)$ is itself expressible in the factorized form,

$$p(ST, \lambda) = \int_0 S(x, \lambda) \cdot T(x, \lambda)\, dx$$

In other words, in terms of a space of ordered pairs $\langle x, \lambda \rangle$ with a product measure defined on it derived from the uniform measure on x and the ρ-measure on λ, we are writing

$$Q(\tilde{S}, \tilde{T}) = \int_\Lambda p(ST, \lambda)\, \rho(\lambda)\, d\lambda$$

$$= \int_0^1 \int_\Lambda S(x, \lambda) \cdot T(x, \lambda)\, \rho(\lambda)\, dx\, d\lambda$$

So factorization has been restored at the $\langle x, \lambda \rangle$ level of description.

Now this is what I understood Shimony to be claiming, that at a suitably refined level of description factorizability holds, and its failure for any given level of description is an indication that that level is not refined enough.

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Your discussion of ~~separable~~ indeterminism seems actually to bear out Shimony's claim, although I take it that you regard your discussion as a counterexample to Shimony's support of Clauser and Horne in linking locality with factorizability.

I am genuinely confused and would appreciate further clarification.

p.20 I am not happy with your discussion of Nelson's theorem. It is ambiguous what you mean by the remark 'each observable $\hat{A}_i$ is made to correspond to some random variable A_i'. If this means that A_i gives the ~~same~~ right probability distribution for $\hat{A}_i$ according to the statistical algorithm of QM, then it follows that

$$\langle \hat{A}_i \rangle_{QM} = \langle A_i \rangle_{h.v.}$$

and

$$\begin{aligned}\langle \hat{S} \rangle_{QM} &= \alpha_1 \langle \hat{A}_1 \rangle_{QM} + \alpha_2 \langle \hat{A}_2 \rangle_{QM} + \cdots \\ &= \alpha_1 \langle A_1 \rangle_{h.v.} + \alpha_2 \langle A_2 \rangle_{h.v.} + \cdots \\ &= \langle \alpha_1 A_1 + \alpha_2 A_2 + \cdots \rangle_{h.v.} \\ &= \langle S \rangle_{h.v.}\end{aligned}$$

But this makes your statement of Nelson's theorem trivially false.

What Nelson did show was, if A_i corresponds to $\tilde{A}_i$ in the sense I have suggested, and if the set $\{A_i\}$ is not pairwise commuting, then there exists a choice of coefficients α_i such that the probability distributions for S and $\tilde{S}$ do not agree (although the expectation values will agree).

What Bell's argument shows, I would have thought, is that random variables that correspond to $\tilde{A}\tilde{B}$, $\tilde{A}\tilde{B}'$, etc cannot be just $A(\lambda)B(\lambda)$, $A(\lambda)B'(\lambda)$, etc., even if the correspondence is restricted to getting only the expectation values right. I fail to see the connection here with Nelson's theorem.

p. 22 ff. I admire the ingenuity of your synchronization and prism models. With regard to the former I feel the term conspiracy model might be more appropriate, if they exactly reproduce QM predictions in all circumstances, and never allow the true 'possessed'

P.T.O

distributions to be probed! But I like your suggestion that synchronization models might actually be experimentally distinguishable from orthodox QM. I am sure this is how progress in the area of correlation experiments will ultimately be made. With regard to the prism models I agree all this is possible and perhaps we must face up to 'desynchronization' in quantum mechanics!

It was a great pleasure to meet you again last summer. May I wish you and your family a Prosperous 1981.

Yours ever
Michael